Philosophical Transactions

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## IV. *Experiments about the Motion of Pendulums* in Vacuo. *By the Reverend Mr* W. Derham, *F. R. S.*

I Have long deſired to know, whether there be any, or what Difference, between the *Vibration of Pendulums in Vacuo*, and the *Air*. And therefore have divers times recommended the Experiment to many ingenious perſons, who had better opportunities of trying it than I. And at laſt, having ſeen *Mr Hawkesbee*'s Experiments in his Pneumatick Engines ſhewn before the R. Society, I accompliſh'd my deſire, by recommending the Experiment to him; who readily undertook it. And having provided himſelf with a proper Receiver, and all other things neceſſary, he, with a Friend of mine in *London*, made the Experiment. The Movements he tryed with were an Eight day Clock vibrating ſeconds, and an Half-ſeconds Movement of mine, which I ſhall ſpeak more of preſently. The iſſue of their Experiment was, My Pendulum vibrated two tenths of an Inch on each ſide farther *in Vacuo*, than it did in the Free Air, and went 7 ſeconds ſlower in 20 minutes, than the other Movement. But in the open Air, my Pendulum in 20 minutes, went only 3 ½ ſeconds ſlower than the other Pendulum.

This Experiment (although I diſtruſted neither the Skill, or Fidelity of the Experimenters) I was deſirous to try over my ſelf. And by *Mr Hawkesbee*'s generous kindneſs, in lending me one of his *Air-Pumps* (which I ſhall always gratefully acknowledge) I was ſoon furniſhed with proper Inſtruments. Which I ſhall give ſome account of, that others may judge how far the Experiment may be relied on, or that it may be better tryed, if mine be found defective.

The firſt Inſtrument was an *Air-Pump* of *Mr Hawkesbee's*, a perſon meritoriouſly known to the R. Society, who I doubt not is the beſt Maker of thoſe kinds of Inſtruments in the World; and his Air-Pumps exceed others in the Eaſe of being pumped, and in the Certainty of their Evacuation; which may be known to a great nicety.

The next Inſtrument was the aforeſaid *Small Movement*, with a Pendulum of about 10 inches, that vibrateth *Half-ſeconds*, and is driven by the Power of a Spring. This Inſtrument I thought commodious, not only for being eaſily fitted with a Receiver, but alſo for vibrating Half-ſeconds very nicely, and alſo becauſe its Vibrations are equal, not ſome large, ſome ſhorter.

The laſt Inſtrument was a very well regulated *Month-piece*, that vibrateth *Seconds* all the Year, with as much exactneſs as moſt do.

Being thus accoutered, the Reſult of many repeated Experiments, day after day, was, That (as before) *in Vacuo* the Vibrations were always larger, than in the Receiver unexhauſted. At the firſt, when my little Movement was newly cleaned, the Vibrations were above $\frac{3}{10}$ of an Inch larger than in the free Air. But afterwards (I ſuppoſe from ſome of the fouled Oyl of the Pump ſpirtled on the Wheels, in letting in the Air, whereby the force of the Spring on the Pallets was blunted, from hence I ſay) as the Vibrations in the unexhauſted Receiver were a little contracted, ſo in the Receiver exhauſted they were more contracted, and only about 0, 25 of an inch larger than in the Free Air.

The *Alteration* in *Time*, which this difference of the Vibrations produced, was conſtantly only about 2 ſeconds in an Hour ſlower, in the Receiver exhauſted, than in it unexhauſted. For if in 4, 5 or more hours going, the two Pendulums did not vary a quarter of a ſecond in the open Air,

Air, or when the Receiver was put over the little Movement, (but unexhauſted ; ) yet when the Receiver was exhauſted, the Half-ſeconds Movement would loſe, at the rate of two ſeconds in every hour, in every Experiment, in many hours going.

And becauſe I was minded to ſee what alterations would ariſe from varying the Vibrations, therefore by opening and ſhutting the Pallets, I cauſed the Vibrations in ſome Experiments to be as large as the Receiver would bear ; in others, to be as ſhort as poſſible ; always adjuſting the Pendulum to vibrate Half-ſeconds nicely in the Air. But ſtill the ſucceſs was much the ſame, or the difference ſcarce perceptible. But only I imagined when the Pendulum vibrated but a little way from the Perpendicular, that the Vibrations *in Vacuo* were not ſo much enlarged, as when it vibrated in a larger Arch.

In all theſe Experiments ( which were repeated divers times with the ſame ſucceſs ) I had no reaſon to move me to think, but that the Vibrations were enlarged *in Vacuo* by the vaſt Rarefaction of the *Medium* : I had I ſay no reaſon but this, That perhaps the different ſtate of the Air might alter the force of the Spring, which drove the Movement. For the tryal of this, I put a well adjuſted *Pocket-Watch* ( with *Hook*'s Regulator, *i. e.* the common ſmall Spiral Spring to the Ballance ) into the *Vacuum*. And after ſeveral tryals, at the ſame pitch of the Spring, I found not the leaſt alteration in the Watches going in many hours ; neither the Springs, nor any other part of the Watch ſeeming to be in the leaſt affected by the *Vacuum* : but the Ballance circumvolving, or keeping the ſame Turns, as in the open Air.

But to be ſtill more ſure, if poſſible ; it came after this into my thoughts, to try what the ſucceſs would be by putting the Half-ſeconds Pendulum again into the Receiver, and only pumping out a part of the Air. And accordingly I left no more Air in, than what kept the included Mercurial

Gauge

Gauge at about 6 inches height. The event of which was, That the Vibrations were then not above $\frac{1}{10}$ inch larger on each ſide, than in the Receiver unexhauſted: and the Time loſt, but about half a ſecond in an hour, or $\frac{3}{4}$ at moſt. And ſo, according as the Mercurial Gauge was more or leſs high, I always found the Vibrations greater or leſſer; they gradually decreaſing, according to the quantity of Air re-admitted.

Having thus given an account of what I experimented, with all the Variety which I thought neceſſary, and with all the care I could, I ſhall for a Concluſion make ſome Remarks, and draw ſome Inferences from what hath been ſaid.

1. What *Mr Boyl* long ſince obſerved (from a Cock'd Piſtol going down as fiercely in his *Vacuum*, as in the Air) may be hereby farther confirmed, *viz.* That the Air is not the Cauſe of the Motion of Reſtitution in Solid Bodies, as Springs. For if it was, it would certainly have been diſcovered in ſo tender an Inſtrument as a well adjuſted Pocket-Watch, lying under the perpetual Influence of two Springs.

2. As *in Vacuo* (where the preſſure of the Atmoſphere is taken off) Heavy Bodies deſcend quicker than they do in the open Air. So it may be obſerved, That Pendulums move ſwifter in the Receiver exhauſted, than in it unexhauſted.

That Heavy Bodies deſcend quicker *in Vacuo,* is evident from the ſwift Deſcent of the leſs heavy Bodies, as *Cork*, the *Down of Sowthiſtles*, the *lightest Feather*, *&c.* which do all precipitantly deſcend, like a Stone, in a tall exhauſted Receiver.

And that the *Pendulum,* in our Experiment, moved faſter *in Vacuo,* is manifeſt from its vibrating but two ſeconds in an hour ſlower, when the Vibrations were $\frac{1}{5}$ of an inch on a ſide, enlarged by the higheſt Rarefaction of the Air. Whereas I find by experiment, that near the ſame encreaſe of the Vibrations, doth, in the open Air, make the Pendulum go 6 or more ſeconds ſlower in an hour. I ſay *near the ſame encreaſe*, becauſe it is ſcarce poſſible to manage the Pallets ſo, as nicely to make the ſame Vibrations as were *in Vacuo.*

The laſt thing I ſhall deduce ſhall be by way of Querey *viz.* Whether the Variations of Pendulums obſerved under the Equinoctial, and between the Tropicks, do not ariſe as much or more from the rarity of the *Medium*, and the encreaſe of the Vibrations conſequent thereupon? It is ſcarce I think to be doubted, but that the Air is much thinner, and finer near the Line, than it is without the Tropicks. And it is evident from the Barometer, that on the tops of high Mountains the Atmoſphere gravitateth leſs than nearer the Center. And therefore (although I like the Notion of the Decreaſe of Gravity from the Encreaſe of the diſtance from the Earth's Center; I ſay, although I like this too well to diſcard it, yet) I am apt to think that this is not the only reaſon of the *Phænomenon*.

I wiſh therefore that *Captain Halley*, when he obſerved at *St Helena* his Clocks to go ſlower than in *England*, had at the ſame obſerved whether the Vibrations were not enlarged. It might be therefore very well worth the while for ſuch as have opportunity, to take notice, Whether their Pendulums between the Tropicks do not make larger Arches than in higher Latitudes? Alſo in what Latitude they begin to alter? Whether the Vibration be greater near the Line, than in any other part between the Tropicks? Or whether the greateſt encreaſe be not always in thoſe places where the Sun paſſeth their *Zenith*? If the Vibrations be found larger under the Line, or in any other part of the Torrid Zone, then it may be obſerved, how much larger they are, and in what proportion they encreaſe, or decreaſe, by approaching nearer unto, or receding from the place of their greateſt Encreaſe?

Alſo it may be worth obſerving, Whether Pendulums do not vary on the tops of high places, or in different ſtates of the Atmoſphere, according as the *Mercury* is high or low in the Barometer? But then in this, and indeed in the former caſes, it is neceſſary, or at leaſt very expedient, that the Movement be ſo exactly well made, that the Power, whether Weight or Spring, do at all times exert the very ſame force upon the Pads or Pallets. Which is very rarely met with. For moſt Clocks are apt to vibrate ſometimes larger, ſometimes leſſer Arches in the 24 hours, according as the Weight or Spring doth more or leſs exert its *forces* on the work.

Thus having given the Society the account which they commanded me; if I have not anſwered their Expectation, yet I have however ſhewed my Obedience to their Commands; and given a Teſtimony of my great Veneration for that Ingenious and Learned Body.